

San Joaquin River Fall-run Chinook Salmon Population Model
External Scientific Review Form

Reviewer: #2

Review:

1. **Problem/Goals.** Is the problem that the project is designed to address adequately described? Are the goals, objectives and hypotheses clearly stated and internally consistent?

Yes. Introduction is very clearly written. Objectives and history are well described

2. **Approach.** Is the approach well designed and appropriate for meeting the objectives of the project as described in the proposal?

No. The regression analysis and model structure does not provide an objective comparison of alternate hypotheses driving smolt production and escapement in the SJR.

3. **Feasibility.** Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives?

The regression analysis is reasonably documented but the analysis has some significant flaws (failure to recognize colinearity among Delta export and Vernalis flow regression parameters).

The model documentation is very poor. The model structure and fitting procedure is very weak, thus the model cannot be used to evaluate alternate policy options. As such its likelihood of 'success' is very low.

4. **Project Performance Evaluation Plan.** Will a monitoring plan be developed to document changes in the restored habitat over time and the response of salmonids and/or riparian vegetation to the restoration in a scientifically rigorous manner?

Not relevant to the report. It is clear, from a scientific/learning perspective, that monitoring of smolt abundance under 5700 and 7000 cfs test flows is required to provide informative contrasts in the data to determine the extent to which flow controls smolt production. How else could one determine whether more water = more fish?

5. **Expected Products/Outcomes.** Are products of value likely from the project?

No. The modeling and analysis is very weak. Vernalis flow may very well be an important limiting factor on the SJR population. However, based on the analysis that is

presented in this report, it remains an open question as to whether Delta exports, Vernalis flow, or both is the key flow-determinant, and the extent to which marine survival, ocean harvest, and freshwater habitat limiting the population. The strength of the author's conclusion that Vernalis flow is the key factor given the ambiguity in the data, and the decisions used in constructing the model, dropping outliers, etc., brings into question the objectivity of the analyst.

Additional Questions:

General:

The purpose of the model is to develop spring flow magnitude, duration, and frequency instream flow levels into the South Delta to adequately protect, and restore, fall-run Chinook salmon in the San Joaquin River basin. To accomplish this objective, please address the topics listed below for these questions:

Is the model adequate?

No. The population model has many flaws:

- It was very difficult to understand the model structure, parameterization, and uncertainty estimates. The non-standard presentation in the text suggests that the author has little experience with population modeling.
- Confusion in terminology makes it very difficult to understand the model structure. For example, the sentence "The first estimation parameter the model predicts is the total number of smolts..." (p. 18). The model predicts changes in state variables like smolt abundance. Smolt abundance is not a parameter. There is no estimation in the model because the parameters are fixed or adjusted by the user. The model simply makes predictions based on 'hardwired' parameter values. This confusion in terminology brings into question whether the author has sufficient background in population modeling.
- The model does not include any density dependence. This is simply ridiculous as it implies that the population can grow to infinite size or should have gone extinct years ago. I am somewhat surprised that the model doesn't show this behavior. I suspect there are a number of ad-hoc traps in the model code to address this problem, which could have implications to policy-relevant predictions. I looked at the spreadsheet model that was supplied. It was very difficult to follow given the plethora of VLOOKUP and IF statements that were part of the model equations.
- The model does not consider other explanatory relationships (e.g. variability in ocean survival, impact of Delta exports) that could be important determinants of smolt production and escapement that would in turn lead to fundamentally different conclusions about the CDFG flow objectives. The model in no way validates or confirms the importance of Vernalis flow since it doesn't allow us to evaluate whether

other explanatory variables can be used to predict the historical escapement pattern. Even if it did, the ad-hoc way of tuning the model, rather than formally fitting the parameters to data using a maximum likelihood approach, doesn't allow for formal evaluation of alternate hypotheses.

- It is unclear how the model was 'tuned' to fit the historical escapement data (p. 22). The parameters that were adjusted were not identified. From looking at the spreadsheet, it appears that both key input data (Vernalis flow by day) and parameter values (e.g. regression slopes) are adjustable. Given so much latitude, it is not surprising that the model can reproduce the escapement trend fairly well. What the author fails to understand is that there are likely many other ways to fit the data just as well (different parameterizations) that would make very different policy predictions. These alternate parameterizations were not explored. Thus, uncertainty in policy-relevant predictions is not determined.

If not, how can model be improved?

The list is long but here are the key issues:

1. Model must allow users to select alternate flow time series (e.g. Delta exports) to drive predictions (not just Vernalis flow).
2. Model must include density dependence.
3. Model should be fit to the data using an objective method (maximum likelihood). This will allow rigorous and objective evaluation of alternate hypotheses and quantification of uncertainty in model predictions.

1. Foundation (justification)
2. Logic
3. Numeric representations
4. Application and reliability
5. Conclusions
6. Calibration and validation
7. Documentation
8. Testing (i.e. what monitoring could occur to validate or reject model predictions)

Specific:

Hydrology

1. Are the methods used in the Model (including Model Report) relating to flow sufficiently documented? If not, what improvements can be made to improve documentation?

No. There is very little documentation on how historic and projected flows at Vernalis are constructed. I am not even sure if the historic, or simulated data was used to calibrate the model.

2. What is the best metric (i.e. arithmetic mean, geometric mean, transformed data etc) that can be developed to adequately capture the variability in spring flow (i.e. magnitude and duration) on an intra-annual basis?

I am not a hydrologist. Consult Maidment. D.R. 1993. Handbook of hydrology. McGraw-Hill Inc.

3. What improvements to hydrologic data utilization can be made to enhance model prediction performance reliability?

As stated above, alternate flow time series should be used as input to the model to determine the extent to which they explain trends in escapement and smolt data.

4. Is there evidence of auto-correlation in flow calculations? If so, what is the affect? Does it need to be removed to improve model prediction (flow determination) reliability? If so, how can it be removed?

For one thing it would be useful to see time series plot of Vernalis Flow, Delta inflows, and Delta Exports on one graph to answer this question. I am concerned that co-variation among these inputs makes it difficult/impossible to tease-out which is the key driver for smolt production. This is not so much an issue of auto-correlation (this is a correlation of values over time) as it is co-variation (correlation of two different variables over time, such as Vernalis flow and Delta exports).

5. Are there additional flow metrics, parameters, logic etc. that should be incorporated into model logic and function? If so, what are they and how can they be assimilated into the model (reference to logic and function)?

Yes. See my comments above for including other flow variables in the model such as Delta exports.

Biology

1. Are the methods used in the Model (including Model Report) relating to fish abundance and/or production sufficiently documented? If not, what improvements can be made to improve documentation?

No. See my comments above on poor documentation and confusion in terminology.

2. What improvements to fish data utilization can be made to enhance model prediction performance reliability?

Doesn't seem like a relevant question to this model as there is no formal habitat suitability component. I don't think I am clear on what this question is getting at.

4. Is there a way to improve how the model performs fish abundance prediction calculations and/or processing of fishery data?

Model does not include any density dependence. See comments on model above and detailed comments below. As far as I could tell from the spreadsheet, it does not remove fish to force the predictions with the historical catch, or to allow one to evaluate the effects of alternate harvesting policies.

5. Is there evidence of auto-correlation in fish related calculations? If so, what is the affect? Does it need to be removed to improve model prediction (flow determination) reliability? If so, how can it be removed?

Auto-correlation will influence stock-recruitment parameter estimates (see Chapter 7 of Hilborn and Walters 1992) through effects of time series bias. However this issue is somewhat moot given the basic mistakes used in analyzing the stock-recruitment data in the report. The stock-recruitment analysis that is presented is extremely rudimentary and violates fundamental principles of population dynamics. Confusion in terms, incorrect interpretations of data, and non-standard assumptions (e.g., linear relationship between escapement and recruitment) suggests the author has little experience with stock-recruitment analysis.

6. Does justification exist to include additional fish metrics, parameters, logic etc. in model logic and function (i.e. ocean harvest and/or Delta export entrainment)? If so, what are they and how can they be numerically assimilated into the model (reference to logic and function)?

This seems to be a leading question, but I agree with it. There is no point in constructing a model that does not include the full range of management alternatives. By only including Vernalis flow and hatchery augmentation, there is no way of evaluating other alternatives like Delta exports or ocean harvest. This basic mistake suggests either that the author has either a very biased perspective, or has little experience with resource management modeling.

7. How can model representation of hatchery production, and underlying model logic, be improved upon?

The details on the hatchery production model were lacking in the document. From what I gather hatchery fish are assumed to have the same reproductive fitness as wild fish when the return to spawn naturally. This is not supported by the literature (see detailed comment below). I also think that hatchery smolts are not subject to flow effects (the numbers reaching the delta are a simple function of the number of broodstock taken). This makes no sense, is inconsistent with the structure assumed for wild fish, and will likely provide an overly optimistic assessment of hatchery benefits.

8. Currently the model predicts a constant ocean survival rate (i.e. relationship between cohort abundance and Chipps Island abundance is constant). Is there a need to make this relationship variable? If so, how can this be numerically accomplished in model performance?

Decadal-scale variation in marine survival rates for Pacific salmon have been well documented and are widely acknowledged to be a very important (if not the most important) determinant of escapement. A stochastic element could easily be added to this type of model, or alternatively, the model could be run at low, medium, and high levels of marine survival.

The policy implications are enormous. Under a reasonable marine survival rate the population may still increase under lower flows. The doubling objective will be met, but it will take longer to attain compared to a scenario with a high marine survival rate. At low marine survival rate, the population may not be sustainable unless freshwater survival rate is improved. The model needs to be able to show these scenarios.

9. The model currently uses an adult replacement ratio of 1:1 as a numerically identified population health barometer. Is there a need to refine this ratio? What additional population parameter(s) could be incorporated into model logic and function?

This term is misused in the text. By definition, the replacement ratio is 1:1. Tracking whether the overall survival rate for each cohort is above 1 seems like a sensible metric as it determines whether the population is sustainable or not. If the ratio is <1 for many years the population will of course decline to low levels. Tracking the abundance of the population in the model will therefore capture the effects of the overall survival rate. Another metric that should be captured is the % of hatchery returns in the escapement.

Statistics

1. Currently the model uses liner relationships between flow and fish production because this relationship provides the strongest correlation value. Is it necessary to include a model toggle switch, model logic, and mathematical functions, that allow users the option to test a variety of non-linear relationships between flow and fish survival and/or production upon model results?

Yes. See my comments above.

2. What is the statistical reliability of model out-put given that model predictions propagate? How can model reliability be improved?

There is no statistical reliability to the model. The tuning procedure is ad-hoc and no objective means of assessing model fit is used. As a result, uncertainty in model predictions cannot be quantified. A maximum likelihood approach should be used to estimate model parameters and quantify uncertainty. These are standard procedures that are commonly used in fisheries stock assessment models.

3. Is colinearity present in model logic and/or computation, and what influence does it have upon model results? If present how can it be removed?

A regression analysis was used to select the most important determinant of SJR escapement. The analysis was flawed from the start by assuming that only a single variable (Vernalis flow, Delta exports, ocean harvest, or escapement) could influence the population, rather than a combination of factors. In addition, trends in ocean survival rate were not considered as one of the explanatory variables, a decision that is very inconsistent with the plethora of literature on effects of marine survival on salmon return rates.

The author failed to substantively recognize the extent of confounding between Delta flows and Vernalis flows on smolt production. The correlation in Delta export and Vernalis flow was not explicitly documented in the report, but the correlation was qualitatively described (p. 14) and can be evaluated (with difficulty) in Fig. 22. Given the regression results, there does not appear to be sufficient contrast to be able to determine which of these factors is the most important to the population. This sharply contrasts with the author's conclusion that Vernalis flow, and not Delta exports, is the key determinant.

4. In some cases, model predictions for salmon production occur outside the empirical data set range used to develop the regression. What limitations in model reliability result?
5. Presently smolt survival has a statistically significant regression correlation with Delta inflow level (i.e. less than 7,000). No statistically significant regression correlation for juvenile smolt survival and Delta export level exists. However when inflow to export ratio is regressed against flow survival, a moderate regression correlation occurs. Currently, exports are not included as a model prediction parameter. Should exports be included as a model prediction parameter (for smolt production)?

YES. See comments above

6. Are the methods used in the Model (including Model Report) relating to statistical evaluations and/or model logic justification sufficiently documented? If not, what improvements can be made to improve documentation?

No. The documentation is poor and does not follow the standard approach of sequentially writing down each equation with symbols used to denote parameters, which are then listed in a separate table along with the assumed or estimated values. The fact that the author has not done this suggests a lack of familiarity with modeling.

7. What improvements to statistical use and application can be made to enhance model prediction performance reliability?

See comments above re. maximum likelihood estimation.

8. There is substantial disagreement amongst scientists regarding the issue of density dependent mortality and its influence upon SJR salmon abundance (e.g. fall spawner abundance and spawning habitat availability: aka redd superimposition). In the absence of flow the relationship between spawner abundance and stock recruit appears to show density dependence (i.e. Beverton-Holt or other density dependent type relationship). However when flow is included with spawner abundance, in the form of a multiple-regression using spawner abundance and spring flow regressed against adult recruits, a significant correlation exists suggesting that density dependence does not explain the variation in SJR adult salmon escapement abundance. How can this issue be resolved with data to date, or in the future if data insufficiency exists currently?

The analysis that is desired is relatively straightforward and described in modern undergraduate fisheries texts (e.g. Hilborn and Walters 1992, See Chapter 7 and p. 294 for a discussion on this exact topic). Density dependence must exist, however there may not be enough contrast in the data to separate out density vs. flow effects if both high densities and high flows occur in the same years. In the analysis that was presented, a linear relationship was used in the abundance/flow relationship. This is nonsensical as it assumes no density dependence at any stock size. This issue could readily be resolved (within the limits of information in the data) by a fisheries scientist with experience in analyzing stock-recruitment data.

9. How can the statistical relationships between flow and fish survival and/or fish production be improved?

I have not reviewed the methods used to determine smolt abundance. Obviously these numbers must be reliable. A relationship between biases in abundance estimates and flow could lead to spurious conclusions about the effects of flow, so I would watch for this when computing smolt abundance data.

The flow-survival relationship parameters should be directly estimated in the model (rather than computed independently via regression) along with other model parameters within a maximum likelihood estimation framework. Under this framework, confounding between the survival-flow relationship and other factors (e.g. escapement) will be apparent when analyzing model output. Most importantly, the increased uncertainty in model predictions that is driven by potential confounding will

be accurately quantified. These approaches have been well documented and are in constant use in resource management and stock assessment fields (see Hilborn and Mangel, 1997, *The ecological detective. Confronting models with data*, Princeton University Press).

Miscellaneous comments:

Figures and tables were difficult to interpret without proper captions.

p. 10, 2nd paragraph. The strong correlation between flow and adult returns does not necessarily imply causality. For example, the high abundance during the mid-1980's (Fig. 1) was a coast wide-phenomena seen from California to BC. It is widely acknowledged as a period of high-marine survival. Flow may have an important influence on Chinook production during some periods, but it is overstating the case to say that production is largely driven by flow. It would be helpful to assign years to the data points in Fig. 3, and to add time series of spring flows at Vernalis, and marine survival (from an adjacent index stock) to Fig. 1.

Figure 1. Does the escapement time series include hatchery contributions?

p. 10, 3rd paragraph. The logic discounting the contribution of fry to future escapements is weak. Item 1 (unknown fry contribution) seems in direct conflict with the main conclusion that fry are not important; 3) is unknown; 4) is irrelevant; and 5) is not substantiated by Fig. 3, which shows the flow-salmon count relationship, not a smolt-adult relationship as the text implies.

Figure 3 (as many others) needs a figure caption. What is meant by the y-axis label "salmon count"? I presume it is the escapement but am unclear on this and whether it includes hatchery returns or not. If it is escapement, why is the maximum escapement in Figure 1 (ca. 70 k) less than the maximum point in Fig. 3 (> 90 k)?

Figure 4-6. Change temperature scale in Fig. 6 to Fahrenheit so the reader can more easily determine the mortality rate at water temperatures shown in Figures 4-5.

p. 13 - last paragraph. Why was ocean survival rate not included as a predictor of Chinook production? The term "Production" should be clarified. I assume it is equivalent to adult escapement.

p. 14 – Delta Exports. The delta-export, delta-inflow, and Vernalis flow – abundance arguments are hard to follow and I think the conclusions are not substantiated. Vernalis flow and export are likely strongly correlated over time (this should be shown) and therefore it is difficult to separate one effect from the other. Although not reported, I suspect that the details of the multiple regression analysis (covariance among Delta and Vernalis flow coefficients) will document this confounding. I am surprised that the survival-export/flow regression has such a low r^2 (Fig. 20). It looks like a fairly strong relationship with one outlier, which is not examined/discussed in the text. Why was only a linear relationship examined in this instance

while in other cases non-linear relationships were used (e.g. Fig. 16-18)? It looks like a non-linear relationship would fit the data quite well. Hence the conclusion that exports are not an important driver for Chinook production seems tenuous and unsubstantiated.

p. 15, 1st paragraph. Further to my point above, the fact that the spring export data do not improve the flow-escapement relationship is not strong evidence that exports are not important. If one reversed the order of the computation, and first regressed exports and salmon abundance, and then asked how much more variance was explained by adding the Vernalis flow variable, I suspect the opposite conclusion would be reached. The order of variable addition should not alter the conclusion. The problem is that the relationships are confounded and there is not enough contrast in the data to sort out the strength of the two effects. Arguments in the 2nd paragraph demonstrate the lack of understanding of this issue by the author. The ratio of two variables (export to flow) will of course decline with an increase in the denominator (flow), but this does not imply that flow is the more important variable. The same issue applies to argument 2). Argument 3) justifies the conclusion based on the flow-production relationship only. Why is no mention made of the export-survival relationship (Fig. 21)? This seems like a very one-sided analysis.

Fig. 19-22. Survival rates should be logit-transformed prior to regressing them on explanatory variables. Note that the models can predict survival rates < 0 or > 1 .

p. 15 – Ocean Harvest. The conclusion that ocean harvest does not influence escapement in the SJR is not substantiated by the data. Figure 1 shows a substantial increase in escapement between 1995 and 2000, a period when harvest rates dropped (Fig. 25). The regression between escapement and harvest index clearly shows that a single relationship is not adequate. There appear to be two negative relationships reflecting recent (lower) and historic (upper) patterns (labeling data points with years would help show this). Note in both cases the relationship is negative implying that either increased harvest reduces escapement (contrary to the author's conclusion), or harvest rates are reduced when escapements are high (as would occur under a fixed catch policy).

p. 16 – In-river Adult Salmon Density. This paragraph shows some serious misunderstandings about stock-recruitment relationships. The confusion is first apparent when the authors conclude that fry density must decrease with spawner abundance if density dependent mortality is occurring. This will only be true at high abundance if over-compensation is occurring. Constant fry abundance (y-axis) with increasing spawner abundance (x-axis) is also indicative of density dependent mortality.

The fact that there is a linear relationship between escapement and fry density does not imply a lack of density dependence for the population. If this were the case the SJR population would either be infinite or would have gone extinct long ago. It is very possible that density dependence occurs after the fry life stage. The policy relevant density-dependent relationships that should be examined are between escapement and smolt production (under the authors untested assumption that fry don't contribute to adult recruitment), and escapement and returning adults.

It is not clear from the stock-recruitment analyses that were done whether hatchery contributions or harvest was accounted for. I am also confused as to why an overall stock-recruitment relationship (escapement vs. cohort abundance after harvest is accounted for) was not presented, with residuals compared to various flow indices. This is the standard way of evaluating density dependent vs. environmental effects as described in basic fisheries text (see Hilborn and Walters 1992). The analyses in Tables 1 and 2 are deeply flawed as they assume no density dependence.

p. 17 – Spring Flow. Again, the strength of the conclusion that spring flows are the key determinant of salmon production is not substantiated by the data. The smolt production-flow relationship (Fig. 29) rests on 2 data points (1995 and 1998). While the data do warrant evaluating production at higher flow levels, the conclusion is overstated. As discussed above, it is highly uncertain whether the key variable is Vernalis flow or export.

p. 18. Smolt production is predicted in part based on a linear relationship with escapement. The model will therefore predict that the carrying capacity of SJR is infinite. Although the model description is difficult to follow, there appears to be no density dependent relationship in the model. It is also not clear why the 1989 smolt ‘outlier’ was removed. I gather because it suggested high smolt abundance with low flow (this outlier should be shown in Fig. 32). The circularity in reasoning is concerning.

p. 18 – Smolt production. It is not clear from the text, whether the model-predicted escapements, or observed escapements, are used to drive smolt predictions. It should be the former, but I suspect the latter. If this is the case then the model structure is deeply flawed (see comment below).

p. 21 – Hatchery augmentation. Hard to follow this. As I understand it, the existing production relationship for the Merced (adults in – adults returned) will be used to drive the simulation. Given this, is hatchery production therefore independent of flow? Are there any competitive impacts on the wild stock? It is also not clear whether hatchery fish contribute to the total number of in-river spawners and therefore subsequent smolt production, or not. If they do, do they have the same fitness as wild-origin spawners? None of these important issues that have been clearly identified in the literature appear to be recognized in the model.

p. 22 - Replacement ratio. This term is misused. The replacement ratio should be the number of spawners required to keep the population at a stable level. As defined in the text, the replacement ratio is simply the ratio of abundance of parents and returning progeny in any year. That ratio may not be sufficient for replacement. The misuse of this term brings into question the author’s familiarity with population dynamics.

p. 22 – Model Constraints. These are not model constraints but desired outcomes. Again, confusion of terminology suggesting lack of familiarity of the subject matter. See Newman and Lindley (2005) for a relevant example of how to document the structure and parameterization of a model.

p. 22 – ‘Validation’. The model was not validated in any way so the title of this paragraph is very misleading.

p. 22 – Uncertainty. It is very unclear how confidence intervals on predictions were computed but I gather that the 95% CL’s for parameters were used in some way. This may be adequate when predicting an outcome from a single relationship, but is nonsensical when employed in a model composed of multiple relationships. Maximum likelihood or Bayesian estimation is the standard approach.

p. 25 – Model Results. The good fit of the model to the observed escapements is very surprising given the simplistic model structure. I can only conclude that the observed escapements are driving smolt predictions. If I have this right, the survival relationships could imply a non-sustainable situation (returns/escapement consistently < 1) yet the population would persist. If this is the case then the model serves no purpose.

p. 25. Fig. 53. The last figure referenced in the text was 41. There is no text or context for the missing figures.

p. 27 – 3rd paragraph. I strongly disagree with the conclusion that this model provides a tool to predict the amount of flow required to meet the doubling goal. The modeling effort violates many basic modeling approaches and biological principles and is deficient on all fronts (structure, parameter estimation, uncertainty analysis, policy evaluation). There are many better (and published) models (e.g. Newman and Lindley 2005, or simpler versions) that could be modified and applied to this problem.

p. 28. Conclusions regarding hatchery augmentation seem dubious. There is no reference to the large literature (e.g. Ford 2002, Nickelson 2003, HRSR 2004) on impacts of hatchery augmentation on wild stocks, especially weak stocks with low intrinsic rates of population growth such as SJR Chinook. The basics of this issue have been well thought out in the primary literature and do not agree with the optimistic or hopeful tone of the conclusion.

References

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